Employment Risk and Household Formation: Evidence from Differences in Firing Costs*

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Abstract

Sharp increases in job security ought to increase the expenditure of durable goods and the likelihood of undertaking hard-to-reverse investments, such as forming a household. We use the large differences in firing costs across contract types in the Spanish labor market to identify if there is a causal link between changes in the risk of job loss and different forms of household formation among youths. Our first identification strategy exploits a legally-induced sharp increase in firing costs 3 years after the starting of a fixed-term contract between 1987 and 1994. The second uses variation in regional incentives to promote high-firing cost contracts between 1997 and 2011. Both strategies suggest that exogenous increases in firing costs, if anything, diminish the probability of forming a new household. Tentative evidence from Household Finance Surveys suggests that higher job security leads youths to accumulate resources to become home owners, thus delaying household formation.

JEL Codes: J1, J2, D91

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Introduction

There are large time and cross-country differences in the rate of household formation among young adults, leading to very different household structure.[1] While in Anglo Saxon and the majority of Scandinavian countries, the fraction of adults between 18 and 35 years of age who live with their parents is below 50%, the corresponding fraction in Southern European countries is about 70%. Regarding time patterns, Bell et al. (2006) use cross-country comparable data that show that coresidence rates have increased since the 80s even in countries where schooling has not increased. Recent evidence from the United States has pointed at a slowdown in the rate of household formation explained by bleak labor markets faced by youths as well as a fragile financial situation (Bleemer et al, 2014). Our study investigates if exposure of young adults to the risk of job loss leads them to postpone household formation.

The pattern of household formation of young adults has important consequences for resource allocation and for public policy. First, understanding whether or not the extended family insures young adults against job insecurity is important to assess how vulnerable workers cope with the risk of falling into poverty (Haider and McGarry, 2005, Card and Lemieux, 2000) and, more generally, to assess to what extent family ties matter for economic outcomes (Alesina and Giuliano, 2008). Second, living with parents confines young adults to focus their job search in a limited geographical area, constraining the quality of the labor matches when search is most effective (Neal, 1999), and diminishing the speed of adjustment to regional shocks. Third, high rates of coresidence put into question the sustainability and the incidence of programs that redistribute income from the young to the elderly. Social Security is in risk when fertility drops, and fertility is lumped with household formation in Southern Europe (Billari et al., 2001). Regarding the incidence of public policies, young adults who live with their parents share consumption of housing and of other goods. Under dynastic resource sharing, intergenerational redistribution from young

[1] We use the term "household formation" to denote the fraction of coresident young adults who leave the house of their parents to form a new household.

to elderly may not be effective as an euro that a young adult passes on to an elderly through a public program may be given back through private resource sharing.[2]

A possible explanation of the incidence of coresidence between young adults and their parents is the increase of job insecurity faced by young adults. On the one hand, the explanation fits the predictions of economic theory: when an expected future income stream becomes more uncertain, agents tend to delay decisions that are costly to reverse. Becker et al. (2007a) present a detailed exposition of that result in the case of household formation. Fogli (2004) delivers a general equilibrium model in which a relationship between employment protection for the elderly and cohabitation between young and old agents arise endogenously. On the other hand, the empirical literature has confirmed a negative link between the employment risk of young adults and the rate of household formation mainly using measures of risk that are aggregated at the regional level. Becker et al. (2007b) document that the probability that a young adult forms a new household is specially high in Italian regions where the stock of young adults hired with fixed-term contracts is diminishing. Gutiérrez-Doménech (2008) finds that Spanish females living in regions where fixed-term contracts are relatively common tend to postpone the decision of marrying. Martínez-Granado and Ruiz-Castillo (2002) document that unemployment rates are positively related with rates of cohabitation.[3]

Our study focuses on the impact of employment risk on the probability that

[2] Recent research has tried to address the question: why do young adults in some countries stay so long with their parents?. Giuliano (2007) highlights the role of culture in shaping the living arrangements of the youth. Rosenzweig and Wolpin (1994) find that low-income children are more likely to live with parents (holding parental income constant). Manacorda and Moretti (2006) document that parents with higher income levels are more likely to live with their children, consistent with the predictions of an exchange model of the extended family. Martins and Villanueva (2009) study the impact of borrowing costs on household formation. We summarize below the literature that measures the impact of employment risk on household formation.

[3] De la Rica and Iza (2005) is an exception as they use individual variation in young adult's contract type and contract conversions to estimate the link between employment instability of the youth and marriage and fertility decisions. We document below that the strong correlation between household formation and contract upgrades does not hold when one instruments contract conversion. Aggregate data also seems to confirm the result. Becker et al. (2007b) and Fogli (2004) also present aggregate evidence consistent with the result.

a young adult forms a new household using contract-specific firing costs in the Spanish labor market. Like Italy, France, Portugal or Sweden, Spain introduced low firing-cost contracts during the eighties, and those were mainly used to hire particular demographic groups, like female and/or young workers (see Dolado et al., 2002). Spanish firms pay higher firing costs upon unilateral termination of permanent (high firing cost) contracts than for finishing fixed-term contracts; between 33 and 45 wage days per year worked vs almost zero at termination if the firm does not renew the temporary contract. Many researchers have documented that workers with jobs regulated by fixed-term (FT) contracts were exposed to higher instability; they faced a much higher probability of transiting into unemployment than those in jobs regulated by permanent contracts (Bover and Gómez, 2004).

We find two advantages for focusing on firing costs. First, while individual perceptions of job insecurity are very useful measures of risk (Manski and Straub, 2000) firing costs can be directly affected by public policy. In addition, legal changes in the Spanish labor markets have indeed changed layoff costs since 1984, affecting identifiable groups of the population and allowing a transparent inference of the impact of changes in the employment stability. Second, relative to measures of job insecurity based on regional unemployment or regional prevalence of low-firing cost contracts, individual-based measures of risk are the closest to the theoretical benchmark, which links the decision of forming a household to the individual situation, rather than to aggregate outcomes.[4] Our main contribution is to estimate the link between a young adult's exposure to the risk of losing the job, measured by low firing cost contracts, and the probability of forming a new household using variation arguably uncorrelated with other variables that determine household formation.

We use two strategies. The first identifies exogenous variation in the type of contract exploiting the legal limit in the time a worker could be hired by a firm

[4] For example, Becker et al. (2007b) estimate the link between *parental* job insecurity and the probability that a young adult forms a new household. They provide very convincing evidence that individual-based measures of risk have a stronger impact on cohabitation than either regionally-averaged unemployment or type of contract does.

using a fixed-term contract. Between 1984 and 1994, firms could only employ a worker under a fixed-term contract (*contrato de fomento del empleo*) for up to 3 years. After that period, the firm was legally obliged to either dismiss the worker or to convert the contract into a permanent one (i.e., high-firing cost one). Contract changes around the period of mandatory conversion of contracts have the advantage of being induced by legal changes, mitigating the influence of confounding factors like more active local labor markets or promotion of better workers. Using data from the 1988-1994 waves of the quarterly Spanish Labor Force Survey (EPA) we provide evidence that the three-year-limit was binding.[5] Either the fraction of workers covered by a permanent contract (the stock) or data on contract upgrades (flows) display a peak in the rate of conversion of fixed-term contracts into permanent ones three-years after starting a labor contract. In a second stage, we examine if three years after starting a labor contract there are discontinuities in either the fraction of young adults who live with their parents or in the flows of household formation.

The second strategy exploits variation across Spanish regions in the introduction and the amount of subsidies to firms to convert fixed-term contracts into permanent ones. Those incentives were introduced in 1997 by different regional authorities as a response of growing rates of fixed-term contracts. Not all regional governments decided to implement subsidies (right-wing regional governments were less likely to do so), and among those regional governments that decided to give subsidies to conversion, the amount of the subsidy varied among groups. Using the 1998-2001 waves of the Spanish Labor Force Survey (EPA) and the 2002-2011 waves of the Spanish Survey of Household Finances (EFF), we construct a sample of young adults first observed living with their parents, and working with a fixed-term contract in the case of EPA and being employees in the EFF. We first document that the mean subsidy in the region (holding regional characteristics constant) was positively related with the rate of conversion of fixed-term contracts into permanent ones using the EPA (and

[5] Similar results are presented in Güell and Petrongolo, 2007 in a study of the determinants of contract changes.

positively related with the fraction of permanent workers in the EFF).[6] We then relate the decision of starting a new household to changes in the type of contract, instrumenting the change in the type of contract with the mean subsidy in the region.

We document three results. First, we do not find changes of the fraction of young adults who live with their parents around the legal limit of three years. Second, we find little evidence of higher rates of household formation three years after signing a contract. Third, neither between 1998 and 2001 or between 2002-2011 do we find differences of household formation between young adults who worked in regions with high or low subsidies to contract upgrades. All those results suggest that young adult's exposure to employment risk, as measured by firing costs, has little impact on their decision to form a new household. Finally, evidence from the Spanish Survey of Household Finances suggests that exogenous increases in job security leads youths to increase mainly their home-ownership rates, but not to form a new household by renting a new accommodation. A possible explanation of our results is that changes in job loss risk alter the patterns of household formation by encouraging some youths to increase renting (thus increasing household formation) and others to staying with their parents to accumulate funds to purchase a new accommodation (thus postponing household formation).

The paper is organized as follows. Section 1 provides some background on the Spanish labor market and on living arrangements. Section 2 lays out the empirical strategy and Section 3 describes the data. Section 4 presents the empirical results and Section 5 examines the patterns of household formation of each type of contract. Section 6 concludes.

[6] García Pérez and Rebollo Sanz (2009) study the impact of the amout of the subsidy on worker flows using administrative data and document similar findings to ours.

1 Fixed-term contracts and living arrangements of the youth

1.1 Living arrangements in Spain

Differences in living arrangements between Southern and Northern Europe have been present for several decades (Billari et al., 2001) and seem to persist even among second-generation migrants in the US (Giuliano, 2007). Yet, as Becker et al. (2007b) discuss, cross-country differences in the rate of household formation have widened since the mid 80s. For example Figure 1 displays that the fraction of Spanish adults between 20 and 34 years of age has increased from 43% in 1987 to 50% in 2002. Figure 1 also shows the growing incidence of fixed-term contracts in that population group during the same period. The resemblance between both time series, together with the basic prediction of economic theory that in the presence of increased risk of losing the job, individuals tend to delay hard-to-reverse decisions, invite investigation of the link between exposure to job loss and delay in the formation of a new household.

1.2 Fixed term contracts: legal framework

Fixed-term contracts were introduced in various European countries as a way of introducing flexibility-at the margin in labor markets with severe firing costs (see Dolado et al., 2002, Güell and Petrongolo 2007). Contracts with low firing costs could be used for new employment relationships while not changing the firing costs of other existing contract types. Spain was the European country with the strongest prevalence of such fixed-term contracts, providing a laboratory to examine the consequences of high exposure to the risk of losing the job.

Fixed-Term contracts featured very low indemnities for termination, that were virtually zero if the firm waited until expiration of the term specified in the contract. Table A.1 summarizes the legal changes with the minimum and maximum periods of duration of temporary contract between 1984 and 1995. During that period, the maximum probation period using a temporary contract was 3 years. After the third year, the firm could choose between finishing the relationship with the worker or signing a high-firing cost ("permanent") contract.

In 1997 a national-wide reform reduced the cost of firing permanent workers from 45 wage-days per year worked to 33 wage-days (see Kugler et al, 2005) At the same time, some of the 17 regional authorities decided to subsidize firms who signed permanent contracts, possibly in response to the growing incidence of fixed-term contracts among vulnerable workers - see García Pérez and Rebollo Sanz (2009), who also examine the impact of those subsidies on labor market flows. Subsidies to contract conversion were typically lump-sum amounts given to firms that proved that a new permanent contract was signed (either by an existing worker whose job was regulated by a fixed-term contract or by a new worker who was unemployed). In some cases, the subsidies took the form of a reduction in the payroll tax . Table A.2, taken from García Pérez and Rebollo Sanz (2009) shows the subsidies by region and demographic group.[7]

1.3 Are fixed-term contracts a good proxy for employment risk?

Workers hired under fixed-term contracts were much more likely to experience a transition to non-employment than comparable workers hired under a permanent contract. Own computations from the Spanish Labor Force Survey suggest that employment risk does vary substantially between both types of workers; while workers covered by a fixed-term contract faced a probability of transiting into unemployment of about 12% in a given quarter, the corresponding probability for a worker covered by a permanent contract was about 3%.[8]

Whether or not workers covered under a fixed-term contract actually *perceive* a higher chance of transiting into unemployment than other workers is less clear. Workers whose job is covered by a permanent contract may perceive

[7] Table A.2 also documents that the size of subsidies varied over time (see the case of Galicia, where subsidies were removed in 1998), and also among demographic groups; Andalucia had special subsidies for firms who changed the contract of workers below 30 years of age into a permanent one. Some regions had higher subsidies for females (Valencia, Cantabria and Navarra, for example).

[8] Regressions of the probability of transiting into unemployment on an indicator of Fixed-Term contract and age, occupation and industry dummies deliver similar results.

that if they lose their job, there are few chances of finding a comparable one, because most exits from unemployment typically happen through fixed-term contracts (see Manski and Straub, 2000). Alternatively, one may claim that some workers may be covered by a fixed-term contract, but still perceive small chances of moving into unemployment due to strong local demand for their skills. We examine the issue using the European Community Household Panel. Figure A.1 documents that indeed one-year changes of worker's satisfaction with job insecurity experience a peak when a fixed-term contract is upgraded to a permanent one. Workers seem to feel more certain about their jobs when these are covered by a permanent contract, rather than by a fixed-term one.

2 The identification strategy

The correlation between contract type and household formation is confounded by several concurrent factors hard to observe, summarized in a footnote.[9] Thus, our study exploits variation in contract type that is weakly correlated with either the ability of the worker or the local labor market. We discuss each source of exogenous variation in contract type below.

2.1 First strategy: Exploiting legal time limits

Our first strategy exploits the discontinuity a legally-induced change of probability of obtaining a permanent contract three years after signing a contract with a firm.[10] Our basic assumption is that as time progresses in a job, infor-

[9] Among other factors, more mature young adults may have a higher taste for independency and be more committed to the labor market, making the firm more likely to be willing to promote the worker with a permanent contract. A simple regression of an indicator of living with parents and type of contract will confound the impact of employment risk with workers' maturity. Second, in a context of limited worker mobility, young adults may face different local labor markets (Topel, 1986). Young adults in better local labor markets may face higher probabilities of being promoted, if they are a relatively more scarce resource, and for the same reason, expect higher future earnings, a factor that is documented to lead to establish a new household.

[10] As mentioned in the 1984 Worker's Act, no worker could work for the same firm for more than three years with a fixed-term contract. Of course, large firms could avoid such regulations by using legal loopholes, like transferring workers covered by a fixed-term contract to other firms of the group when the three-year limit binds. All we need for our instrument to work (i.e., to predict contract changes) is that transferring workers within the same firm carries a cost that makes it more likely for firms to upgrade the contract. Loss of position-specific

mation about a worker between 20 and 34 years of age, experience and outside options accumulates continuously. A young worker who has been working for a firm for two years and eleven months has revealed virtually the same kind of information to the market as a young worker that has been working in his or her position for three years and one month. Nevertheless, there is an unambiguous mean increase in job security. After three years, firms have an incentive to convert some contracts into high firing cost ones if they want to retain specific workers.[11] Thus, the pool of workers three years after having started working with a firm will contain workers who got this permanent contract, and another set that will be essentially in the same situation as before those three years. The reason is that those workers still can get a contract in a new firm covered by a fixed-term contract; Bover and Gomez (2004) document that the main way out of unemployment is through a fixed-term contract.[12] Our methods exploit that discontinuity in the exposure to job insecurity among the pool of workers to examine the impact of employment security on living arrangements.

Two reasons lead us not to use actual time spent in the firm as our "running" variable in the regression discontinuity design. First, focusing on "actual" time spent with the firm may lead us to analyze the selected group of workers who stay in a particular firm three years (those that the firm is specially interested in). Second, the characteristics firms use to retain fixed-term workers during the three-year period may be correlated with the propensity to establish a new household (higher-ability workers with higher income prospects). Instead, we construct a new variable called "potential tenure" (t_i). The variable keeps track of the time elapsed since a contract was first signed with a firm, *regardless* of whether or not the worker keeps on working with the same firm. Our strategy only exploits exposure to the option associated to the time limit, but not the

human capital may be one example of such costs.

[11] The incentive may not only be ability-related, as there may be temporary shortages of a specific type of human capital or skill level that leads firms to hire workers with a high firing cost contract.

[12] While a regulation impeded firms from contracting a worker under an FT contract if he or she had already worked three years wih another firm, it is not clear at all that such rule was enforced.

actual take-up of the option of getting a high firing cost contract.

2.1.1 Specification in levels

We start by relating the two outcome variables (whether or not the young adult lives with parents and the type of contract he or she is covered by) to time since any contract started signed. We use data on young adults (between 20 and 34 years of age) to estimate separate regressions of the form.

$$Y_i = \alpha_0 + \alpha_1 TREAT_i + f(\widetilde{t}_i) + \varepsilon_{it} \tag{1}$$

Y_{it} denotes working with a high firing cost contract or a dummy variable that takes value 1 if the young adult lives with his or her parents, depending on the regression. $PTEN_i$ denotes the time that has passed since the worker first signed a FTC with the form. The key variable in (1) is $TREAT_i$,a dummy indicating whether or not more than three years have passed since a worker started working in a firm: $TREAT_i = 1(t_i \geq 3)$. Ideally, we would like to run this regression for a sample of workers with similar initial conditions: those who signed a FTC in the same period and then track changes in the contract they hold as they approach the three-year limit. In addition, one would like to know the number of days the worker has been working with the firm.

In practice, our sample follows individuals for one year and a half, so to track all the conversion patterns around the 3-year limit, we use data on workers who already have spent some period at the firm once they entered the survey. That is, for some workers, we use observations on actual time spent by the firm. That is, we observe

$$Y_i = \alpha_0^j + \alpha_1 TREAT_i + f_j(j + t_i) + \varepsilon_i^j \quad 0 \leq \widetilde{t}_i \leq 1.5 \quad 0 \leq j \leq 4 \tag{2}$$

j indexes the quarter of actual time spent at the firm when the worker entered the survey, and $\widetilde{t}_i$ is the number of quarters that we observe the young adult in the sample. Note that $f_j(j + \widetilde{t}_i)$ depends on j because the amount of information revealed to the firm about the productivity of the worker that is revealed may change with actual time at the firm. We estimate model (2) stacking all initial durations. The implicit assumption we are making here is that the impact of time spent at the firm on the outcomes of interest is a smooth function of the initial observed tenure when the worker entered the survey.

$$Y_i = \beta_0 + \beta_1 TREAT_i + g_1(j+t_i) + g_2(j+t_i) * TREAT_i + u_i \quad 0 \leq t_i \leq 1.5 \quad 0 \leq j \leq 4 \tag{3}$$

Given the discreteness of t_i, we model $f()$, and $g()$ using global polynomials of potential tenure. Those functions capture the impact of any variable that changes continuously with time elapsed since the contract was signed. The order of the polynomial in model (3) is determined using a specification test in Lee and Card (2008).[13]

Coefficients of interest We first assess to what extent there firms complied with the 3-year limit policy. To that end, we estimate the intention-to-treat parameter β_1 in a model where "working with a firing cost contract" is the dependent variable. β_1 measures a discrete jump in the fraction of workers covered by a "high firing cost contract" 3 years after starting any type of contract. The policy could be ineffective because virtually all useful information is revealed already by the third year of the relationship or because the special contract with the 3-year limit was barely used by firms. Alternatively, a maximum legal period could prove ineffective because (large) firms could transfer workers to other subsidiaries using another a new contract and thus avoid converting low-firing cost contracts into "high firing cost" ones around the 3-year limit. In all those cases there would be no discrete jump in the stock of "high firing cost"

[13] Lee and Card (2008) show that the ratio

$$\frac{(SSR\_restr - SSR\_unrestr)/(J-K)}{(SSR\_unrestr)/(N-J)}$$

follows an F(J-K,N-J). SSR_restr is the sum of squares of the residuals of model (1), $SSR\_unrestr$ is the sum of the square of the residuals of the dependent variable on an unrestricted set of dummies with each possible value of t. J is the number of cells (13 when the window is 1.5 years before and after the 3-year limit) and K is the number of terms of the polynomial (including the dummy and interactions with TREAT).

contracts 3 years after beginning to work with a firm, and β_1 would be zero.

Secondly, we assess whether or not the coefficient of $TREAT_i$ in a regression that has "living with parents" as the dependent variable. β_1 is an intention-to-treat parameter that measures the impact of the discontinuity in the incentive to convert a contract into permanent on the living arrangements of young adults. If exposure to the risk of going into non-employment does affect the probability that a young adult forms a new household, the coefficient would be negative and significantly different from zero.[14]

Functional form: We follow most of the literature on Regression Discontinuity Design in estimating (1) using OLS.[15] Note that this is not such a strong functional form assumption as it may seem at face value. The (weighted-by potential-tenure cell size) OLS estimates of model (3) without including covariates other than the polynomial in "potential tenure" and the variable $TREAT$ are identical to OLS estimates with the dependent variable aggregated at each value of the variable "potential tenure". In an aggregated regression, the dependent variable is continuous (censored between 0 and 1), so the usual concerns about linear probability models in individual-level data making predictions outside the 0-1 range or yielding marginal effects difficult to interpret can be directly judged from the graphs showing the estimated relationship between "potential tenure" and the outcome of interest: fraction of young adults whose job is covered by a "high firing cost" contract and fraction of young adults who live with their parents. While our preferred specification will mainly rely on linear probability models, for the sake of completeness, we will also present results using a Probit specification.

Standard errors: in all specifications are clustered at the level of quarters elapsed since a contract is signed.

[14] A concern with this strategy is that the changes from FT to permanent contracts could be associated with wage increases (see De la Rica, 2003). Thus α_1 could also pick up wage increases associated to contract changes. As wage increases are positively related to household formation in virtually any paper on coresidence we are aware of, α_1 is most likely to be an upper bound on the impact of a fall in employment risk on household formation (see Rosenzweig and Wolpin, 1994; Aasve et al., 2002, Haider and McGarry, 2005, among others).

[15] See Angrist and Lavy (1999) or Van der Klaauw (2002)

2.1.2 Specification in flows

To examine more closely the dynamics of the household formation and contract conversion, we also perform an analysis of flows. We use a sample of young adults who, in the first observation in the sample live with their parents and work covered by a fixed-term contract. We then examine the rate of leaving each of those situations. We thus estimate the following regressions:

$$\Delta Y_i = \beta_0 + \beta_1 TREAT_i^1 + f_1(j + t_i) + u_{it}^1 \tag{2}$$

ΔY_i is a binary variable that takes value 1 if young adult i has a high firing cost contract in quarter t but not in quarter $t-1$. $TREAT_{it}^1$ is defined as a dummy that takes value 1 in all the quarters between the third and 4 year. Strictly speaking, model (2), should only include a dummy taking value 1 in the first quarter after the third year, because that is the period when the legal limit binds. We discuss the issue in the following section. $TREAT_{it}^1$ is again an intention-to-treat parameter that measures the impact of any event that happens in the third year after a worker signs a fixed-term contract with a firm. In model (3), we use a variable that takes value 1 for four quarters to allow for some lags in the impact of contract conversions on household formation, as the latter can be a slow process.

The coefficient of interest is γ_1, that measures the impact of any development between 3 and 4 years after a fixed-term contract is signed on new household formation (above and beyond of what a smooth function of time would predict). γ_1 would be positive if a contract with higher firing costs reduces employment risk and young adults react to higher security by establishing a new household.

Finally, one concern about models (2) and (3) is that they are estimated on selected samples; we discard young adults whom we do not observe living with their parents, who are not working or who are covered by a permanent contract. In principle, such initial sample selection biases will not affect the coefficient of $TREAT_{it}^1$ if they are a smooth function of time passed since signing a fixed-term contract.

2.2 Second methodology: regional variation in subsidies to contract conversion

Our second strategy uses the regional variation in incentives to convert fixed-term contracts into permanent ones documented in Section 2 as a source of identification. Basically, we assume that the evolution over time of those subsidies is uncorrelated with decisions of household formation for channels other than the conversion of a temporary contract into a permanent one.[16]

We focus the analysis on a sample of young adults in the Spanish Labor Force Survey whom we observe living with their parents, working on a job covered by a fixed-term contract. We measure the causal impact of a change of contract from fixed-term into permanent, using the following bivariate probit of the probability of forming a new household as a function of the contract type.

$$new\_hh_{it} = 1[\delta_0 + \delta_1 \Delta perm_{it} + \beta X_{it} + e_{it} > 0] \tag{4}$$

$$\Delta perm_{it} = 1[\gamma_0 + \gamma_2 subsidy + \gamma_1 X_{it} + e^1_{it} > 0] \tag{5}$$

new_hh_{it} is a binary variable that takes a value of 1 if we observe the young adult establishing a new household in that quarter and 0 otherwise. $\Delta perm_{it}$ is a binary variable that takes a value of 1 if we observe the individual's contract changing from temporary into permanent, and zero otherwise. Again, as it is rather unlikely to find an instantaneous impact of contract conversion on household formation, we set $\Delta perm_{it}$ to 1 in any observation following the conversion of a fixed-term contract into a permanent one. X_{it} contains region dummies, the age of the young adult, family size in the original household, year dummies and quarter dummies and the regional unemployment rate of the first quarter when we observe the individual.

[16] Two notes are in order. First, as mentioned above, the introduction of those subsidies coincided with a major, national-wide reform that diminished firing costs for workers who were employed under a permanent contract. To avoid problems with the increase in employment risk among workers with permanent contracts, we have chosen to focus the analysis on the post national-wide reform. Second, large regions like Catalonia and Madrid decided not to implement those subsidies in 1998. There is some concern about the endogeneity of adoption, that we try to address below.

The key variable identifying the system of equation is *subsidy*, that measures the economic incentive a firm in a given region in a given year faces to upgrade a contract from fixed-term into permanent and is measured in thousands of 1995 euro. We do not observe if the firm for which the young adult works actually got the subsidy, and use only the variation in subsidy that is presented in Table A.1. e_{it}, e_{it}^1 are random disturbances, distributed as a bivariate normal with mean zero. To allow for a lagged effects of contract upgrades on household formation, we keep one observation per individual, and the outcome variables take value 1 if we see any transition in any given quarter during each spell.

The parameter of interest in this specification is δ_1, that measures the causal impact of a decrease in the probability of losing the job on the propensity to establish a new household. The system of equations (1) and (2) is identified by the joint normality of the error terms and by the assumption that the regional amount of the subsidy only affects the probability that a young adult forms a new household through its impact on the propensity of the firm to sign a new contract.

3 Data

We use the 1988-2001 waves of the quarterly Spanish Labor Force Survey (*Encuesta de Población Activa* or EPA). The EPA is a rotating panel that tracks individuals for up to six periods, aside from attrition issues. EPA contains information about the labor market status of each individual, and its relationship to the head of the household. It also contains information on age (in five year bands), occupation, industry and schooling.

3.1 Samples used for the first strategy, exploiting legal time limits

Stock sample We use a sample drawn from the waves between the first quarter of 1988 and the fourth quarter of 1994. The reason for this time limits is that prior to 1987, the survey contained no information about time spent at the current firm. In addition, we dropped the year 1987 because fixed-term contracts were introduced in 1984, and by 1987 only a small amount of contracts were converted because of the 3-year rule. Finally, we do not use any wave after the fourth quarter of 1994 because the 3-year rule was effectively stopped in 1994.

For that sample, we construct the variable "potential tenure". As mentioned above, a problem with the potential tenure measure is that the Spanish Employment Survey does not contain monthly-level information on tenure, but only on years. To construct the time passed since the contract was signed, we use a strategy also used in Güell and Petrongolo (2007). Whenever an individual enters the sample, if tenure at the firm is one, two, three or four years, we assign a quarter from an uniform distribution with four values: 0,1,2,3. We then accumulate potential experience using the first observation on actual tenure in the present firm we observe. Every quarter, we add .25 to our measure, regardless of the situation of the young adult in the labor market. Now, by not using the exact running variable, but an imputation, some of the advantages of the regression discontinuity methodology are lost. We thus experiment with alternative measures of time elapsed since the FT was signed and report the results in a footnote.[17]

Finally, by construction, our measures of "actual" and "potential" tenure will coincide if the individual stays with the same firm. Otherwise, actual tenure will always fall below potential tenure.

Flow sample For some of the specifications, we need to define the variable "establishing a new household." Such event is hard to measure, as the EPA does not track young individuals who leave households in the interview to establish a new one. We follow Martins and Villanueva (2009) who define that a young adult has left the sample if, conditional on the original parental household being in the sample in quarters q and $q+1$, we observe the young adult as a household

[17] Namely, the strategy has been the following. Tracking young workers for 6 quarters, we use changes in the report of when actual tenure at the firm passes from 1 to 2 years (or from 2 to 3, 3 to 4 and so on) to infer the number of quarters the worker had been working with the firm in the first observation. We have then re-computed the "potential tenure" measure. The results are similar to those shown, and are available upon request.

member in period q but not in period $q+1$.

Linking individuals and households over time is not an obvious task. The Spanish Statistical Agency provides two versions of EPA. The first is a series of cross-sections with detailed individual and family information, but in which all identifiers that would allow tracking an individual over time are scrapped before releasing the data. The second is a longitudinal file in which the information that allows tracking an individual over time is released, but all the identifiers that identify the household the individual belongs to are scrapped before releasing the data. Thus, to construct measures of household formation in the longitudinal version of the EPA one needs to identify the household an observation belongs to. We have benefitted from the invaluable help of Ildefonso Mendez, who has developed a software (using strictly public information) that allows identifying individuals belonging to the same household. We have done some checks to ensure that our matching made sense, like cross-checking the number of families reported by our matching and those in the cross-section version of the EPA, and checking if the number of household members provided by our measure is not larger than the number of family members reported by the EPA.

Below, we compare our measure of rates of household formation among young adults obtained using panels that do track individuals as they leave their original household. Our sample is restricted to young individuals who are between 20 and 34 years of age and who are working. We exclude young adults who are unemployed in the first quarter we observe them, as we assume they are not exposed to the risk of losing their job.

The summary statistics of the flow and stock samples are presented in Table 1.

3.2 Sample used for the second strategy, using regional variation in subsidies to conversion

The second sample is described in Table 2. A key variable in this second strategy is the amount of the subsidy that a firm qualifies for after converting a transitory contract into a permanent one. We ignore much higher subsidies for contracting an unemployed person as a permanent worker because flows from unemployment to employment basically occurred through FT contracts. Table 2 presents summary statistics of subsidies, quarterly rates of contract conversion (from fixed-term into permanent) and quarterly rates of household formation. The rates of household formation range between 1.4 percent and 2.8 percent per quarter.[18] Regions with a higher fraction of permanent contracts tended to have lower subsidies (see the case of Catalonia, Madrid or Baleares). Thus, we have also experimented controlling for regional unemployment rate pre-1998, with little impact on the results.

4 Results

Table 3 presents OLS regressions of the event "establishing a new household" on the variable "the contract changes from transitory into permanent", holding constant the age, region, gender, year and the regional housing price. The coefficient of "contract changed into permanent" in model 1 is .0057 and implies that once a young worker living in a two-person family and between 26 and 30 years of age obtains a permanent contract, his or her chances of establishing a new household in the following quarters increase from 2.2 percent to 2.77 percent. Model 3 distinguishes between the events "contract upgraded in this quarter", "contract upgraded last quarter" and "contract upgraded two quarters ago". The pattern of results suggest that most of the statistical association between both variables happens in the quarter of contract conversion and the next one. As mentioned above, the results in Table 3 cannot readily be interpreted as a causal impact of employment risk on household formation.

4.1 Results using legal limits to conversion

We start by illustrating our empirical strategy to detect a causal impact of employment stability on household formation among young adults in Figures 2 and 3. We examine if "bunching" in contract upgrades (from fixed-term into

[18] The yearly rate of household formation for the same period estimated in Martins and Villanueva (2009) using the European Community Household Panel is 8 percent.

permanent ones) three years after a labor relationship is started is mirrored by a discontinuous change in the patterns of household formation.

Figure 2 displays the quarterly pattern of contract types between one and a half years and four and a half years after the moment a worker starts a relationship with a firm. The actual fraction of permanent contracts is shown as dot points, and the full line is the predicted value from an OLS regression of a binary variable taking value 1 if the contract is permanent on a third-order global polynomial of time elapsed since the contract was signed, the $TREAT_{it}$ variable and a third-order polynomial of potential tenure if it exceeds 3 years. The regression is weighted by the number of observation in each quarter-cell. Possibly due to the accumulation of experience in the labor market, firm's learning about the ability of the worker, and to a speedier rate of arrival of job offers as experience is accumulated in the labor market, the time profile of contract conversion is positive both before and after the three year limit. The rate of increase in the fraction of contracts with high firing costs is relatively high one year after signing an FT contract, but diminishes two years after signing a FT contract. At the three-year limit, we detect a 4.6 percent jump in the fraction of contract conversions. The discrete jump is clearly out of the 95 confidence bands of the predictions at either side of the discontinuity. An interpretation of that pattern is that employers delay contract upgrades until they are legally mandatory. Note also that the actual fraction of permanent contracts one quarter before the three-year limit lies exactly on the regression line in Figure 2, indicating that the third order specification of the polynomial for the regression before the three-year limit leads to small specification error (see Lee and Card, 2008). Figure 3 displays exactly the same predicted values for regressions where the dependent variable is whether or not a young adult lives with his or her parents. At the 3-year limit, while we detect a drop in the fraction of young adults who live with their parents (less than a percentage point), the estimate is not significantly different from zero. Once we concentrate on the population of 25-34 years of age, when emancipations are most common, the drop disappears (Figure 4).

Table 4, Panel A shows selected estimates of the coefficients of model (1), with the dependent variable being the type of contract (1 if permanent, 0 if fixed-term).[19] The coefficient of interest is the intervention dummy $TREAT_{it}$, indicating whether or not more than three years have passed since the INITIAL FT contract was signed. We present results for two window sizes: the first is 2 and a half years before and after the three year limit (columns 1-2). The second is for 1 and a half year the three year limit. The magnitude of the estimate in Model 1 of Table 4 is .036. That means an estimated "jump" of contract conversions of 3.6 percentage points at the legal limit of three years. The magnitude of the "jump" in contract conversions ranges between 2.6 and 4.3 percent in Models (1)-(5), depending on the sample size and the window limit. Table 4, Panel B shows the coefficients of the intervention dummy on a regression with the dependent variable being "the young adult lives with his or her parents", and otherwise identical to that shown above. For all sample splits, the estimated coefficients are small, positive and statistically not different from zero. A positive estimate implies that the fraction of young adults living with parents *increases* following an increase in job stability. Thus, we fail to detect a change in the living arrangements of young adults around the time of mandatory contract conversion.

4.1.1 Evidence from conversion rates

Next, we explore the dynamics of contract conversion and new household formation. We illustrate our strategy in Figures 5, 6 and 7. Figure 5 displays average contract conversion rates by quarter elapsed since a FTC was signed in dot points. The full line denotes the predicted value from a linear regression of contract upgrades on time passed since the contract was signed, a dummy

[19] The standard errors are clustered at each level of year elapsed since contract was first signed with the firm (see Card and Lee, 2006). Additional covariates (not shown) are year dummies (1990 omitted), a dummy for whether the young adult is between 20 and 24 years of age and another dummy for 25-29 years of age, 1-digit industry dummies, three dummies of educational attainment ("does not read", "primary school" and "professional training") and region dummies (Andalusia is omitted). Note that we do not control for covariates like family size in this specification due to its mechanical correlation with the dependent variable "the child lives with parents".

taking value 1 between 3 and 4 years, a linear term on time elapsed if it is larger than three years and an additional linear term of time passed if greater than four years. We document a discontinuity at the three-year legal limit when upgrades to permanent contracts become more frequent. Now, conversions are higher not only in the first quarter of the third year, but during the four quarters between the third and the fourth years. We should only observe a peak in the third year, first quarter, which raises the issue of whether the abnormal conversion rates are indeed due to the regulation or to other information revealed during the third year.

To get further evidence about the nature of such exceptionally high conversion rates throughout the fourth year after signing a FTC, we plot in Figure 6 the corresponding estimates during the period spanning 1995 and 1998, when the 3-year limit was eliminated for most fixed-term contracts. The plot in Figure 6 suggests that there was neither a discontinuity at the three year limit nor a period of abnormal contract conversion rates throughout the third year. With some caveats, we thus infer that the higher conversion rates all through the fourth year of "potential tenure" between 1988 and 1994 are due to the existence of a three-year limit.

Next, to detect in a simple way if there is a change in either contract conversion or in living arrangements at any particular time after signing a fixed term contract, we fit a first-order global time polynomial in time elapsed since signing a contract to each of the variables of interest: contract conversion and formation of a new household through youth emancipation. We plot the residuals of each of those regressions averaged by quarter elapsed since the contract started in Figure 7. The full line in Figure 7 connects the residuals from the contract conversion equation, and the dotted line those of the household formation equation. While the series are very close to each other up to the year 1.75 (i.e., quarter 7 after contract conversion), the residuals of the contract change equation become positive and rather stretched between 3 and 4 years after signing the FTC, suggesting that there is some event at year 3 that leads to more conversions. Conversely, the series of residuals of new household formation does not exhibit such stretching at three years. If anything, household formation seems to *lead* contract conversion.

Table 5 show the estimates of equations (2) and (3). We choose to work with an intervention variable $TREAT$ that takes value 1 during the four quarters of the third year, instead of only the first one for the reasons stated above.[20] Finally, instead of dropping observations in which a young worker has had his or her contract upgraded into permanent in the past, we still maintain those observations in the sample with a value of the dependent variable in the contract conversion regression of 1. The top panel of Table 5 presents OLS estimates of equation (3), when $f()$ and $g()$ are quadratic functions of "potential tenure". In all cases, the dummy indicating that between 3 and 4 years have elapsed after signing the contract is large and positive: conversion rates increase by between 3 and 5 percentage points during such period. The estimates are significantly different from zero for usual confidence levels (the standard errors in this case are adjusted for correlation between the observations belonging to the same individual).[21] The bottom panel of Table 5 presents OLS estimates of the link between household formation and the intervention dummy. The coefficient has the wrong sign; contract conversion impacts negatively household formation. Nevertheless the precision in our preferred sample (males, 25-34 years of age) is small.

4.2 Regional variation in subsidies to convert temporary contracts into permanent ones

We turn to an alternative source of identification: the incidence of regional subsidies to contract conversion between 1998 and 2001. One could always

[20] The results are somewhat sensitive (in terms of precission) when we specify two or three quarters after the third year). We think that the randomization of the number of quarters biases the results against finding bunching at exactly 3 years in contract conversions. Due to lags in household formation, we find it less plausible that the measurement error in our intervention dummy obscures the coefficient of in the specification with new household formation as a dependent variable.

[21] In the specifications with flows, we found standard errors to be much more sensitive to assumptions about correlations across the same individual than to assumptions about correlations across potential tenure levels. For that reason, the standard errors in Tables 4 and 5 are clustered at the individual, rather than at the tenure, level.

argue that evidence based on legal time limits applies to a particular subset of the population: young adults who get an upgrade of their contracts only because of the existence of a time limit (possibly the less motivated youth). Evidence from other (local) impacts in the same direction would reassure us about the (absence of) a link between job security and household formation.

We start by examining whether regional differences in the subsidies different regions (as published in official regional bulletins) explain regional differences in patterns of household formation. Table 6 presents Probit regressions in which the dependent variable takes value 1 if the temporary contract is observed changing into a permanent one. Those are first stage regressions that test the validity of the instrument (amount of the subsidy) in explaining contract conversions.[22] As before, we experiment with different subsamples. The first contains all young adults whom we first observe living with their parents and with a temporary contract, and are between 20 and 34 years of age. The second subsample focuses on young adults between 25 and 34 years. Finally, we split the sample by gender. All regressions include controls for age, gender, region, industry, occupation and schooling, as well as some parental characteristics, like family size. Standard errors are computed assuming arbitrary correlation among observations belonging to the same age and region group.

The coefficients in the first panel of Table 6 reflect the marginal change in the probability of conversion when one changes the variable of interest by a unit, holding the rest of the covariates constant. In all models, but in males between 20 and 34 years of age, larger subsidies result in higher conversion rates. The coefficient measuring the impact of the subsidy variable varies across specifications, and denotes that an increase in the subsidy by 1,000 euro (in euros of 1995) increases contract upgrades by about 3 percentage points. As subsidies in the region may be correlated with other characteristics that affect both contract conversion and the labor market performance of young adults, we examine whether subsidies explain pre-1997 conversions in Table A.3. The

[22] The specification in this strategy focuses on "flows" (individual changes in the type of contract or living arrangements), because we did not get convincing evidence that subsidies changed the stock of young adults with permanent contracts.

OLS coefficient of the variable *subsidy* reported in Table A.3 are positive, much smaller than those in Table 6, and not significantly different from zero.

The coefficients in the second panel of Table 6 show intention-to-treat impacts of the amount of the subsidy on the probability of forming a new household. As in the previous specification, we allow for lags between contract upgrades and household formation. Namely, we collapse all observations belonging to the same individual and build indicators of "formed a household" and keep the information of the subsidy in the first quarter the young adult is observed. The coefficients measuring the impact of the subsidy on household formation is negative and small: 1,000 extra euros *diminish* the chances of forming a household by a third of a percentage point.

Table 7 presents estimates of the link between contract conversion and household formation using a Bivariate Probit. The estimates are negative. The most precise estimate is shown in Table 7, row 1, column 1 and suggests that a contract diminishes the chances of household formation by 2.8 percentage points. Even adding two standard errors to the point estimate, we obtain that the extreme of the 95 percent confidence interval is .004, below the estimates in Table 3. Once we use smaller samples, the standard errors become too large to draw credible inferences, but the qualitative results do not change. Overall we infer from the second strategy that the type of contract does not seem to have an important causal impact on the living arrangements of the youth.

5 Discussion: Patterns of household formation

This section investigates if job insecurity affects the form of household formation, rather than the probability of leaving the parental household. The evidence comes from the 1994-2001 waves of the European Community Household Panel, a survey much more comprehensive than the Spanish Labor Force Survey and that does track young adults as they form a new household. Unfortunately, the information in the survey does not permit us to construct our key instruments (the 3-year rule was not binding between 1995-2001, and we lack information on region to construct the variable "subsidy"). Therefore, the evidence presented in this subsection is mainly suggestive.

Table 8 provides additional evidence about the link between low-firing costs contracts and household formation on one hand and the type of housing demand on the other. The first column in Table 8 shows the rate of household formation among young workers in four European countries where fixed-term contracts are widespread; Spain, Italy, France and Portugal. The first column in Table 8 documents household formation rates by type of contract. The second through fourth columns show the fraction of young adults who establish a new household distinguishing by three routes of tenure: owner, renter and rent-free accommodation. As before, we find that Spanish workers with permanent contracts are more likely to form a household (.077 per year) than those covered by a FTC (.065). The first column displays a contract-related gap in household formation only in Portugal (Panel D, .068 for permanent contracts vs. .059 for fixed-term contracts), but neither in Italy or France.

Second, we interpret from columns (2)-(4) of Table 8 that what fixed-term contracts may affect the type of tenure arrangement in the first accommodation. The fraction of Spanish young workers covered by a high-firing cost contract who form a new household by purchasing their accommodation is 69% (the ratio between the rate of household formation as owner: .053 Table 8, Panel A, column 2 row 1 and the overall rate of .077), while only 20% rent the house they move to (the ratio of household formation as renters, .016 Table 8, Panel A, column 3, row 1 and .077). Conversely, among young workers who form a household and are covered by a fixed-term contract, the choice of renting is much more common; 32% of Spanish workers who establish a new household and have a fixed term contract rent their new accommodation. France and Italy exhibit a qualitatively similar pattern; workers covered by FT contracts have a relatively higher propensity to rent a new accommodation than to purchase one.[23]

[23] Another finding (not reported) is that, in all countries, workers with fixed-term contracts are less likely to become home owners paying a mortgage loan than young workers with a permanent contract. An interpretation is that the impact of firing costs on tenure arrangements

Overall, our interpretation of the evidence in Table 8 is that young adults may react to the presence of employment risk by choosing routes of household formation that involve a lower adjustment costs in the case of an income drop. Owning involves large adjustment costs in face of an income downturn; selling a house is a costlier process than leaving a rented accommodation.

Causal estimates of job insecurity on the housing tenure regime To investigate further if job security affects the housing tenure regime that young workers choose to live when they form a household independent of their parents, we show causal evidence from the 2002-2011 waves of the Spanish Survey of Household Finances (in Spanish, *Encuesta Financiera de las Familias*, EFF). This is a triennial survey conducted by the Banco de España, which interviews around 6,000 households and obtains detailed information about their wealth holdings, debt and consumption, as well as individual information about personal characteristics, earnings and labor status. Using the EFF survey we construct a stock sample of household members between 20 and 40 years, who are employees and who earned at least 1,000 euros in 2005 constant terms in the year prior to that of the survey interview. Job security is measured by the kind of job contract the young workers hold, whether a permanent contract or a fixed-term contract. As the kind of job contract is an endogenous variable, we obtain causal estimates by instrumenting the stock of permanent workers with the mean regional subsidies that firms were eligible for to convert fixed-term contracts into open-ended ones (permanent contracts) in period 1997-2009. The variable of regional subsidies is expressed in thousand euros of 2005 using regional deflators of the gross household disposable income. We follow a similar estimation strategy to that carried out in Barceló and Villanueva (2016) to estimate the household wealth response to the risk of job loss. To study the impact of job security on the housing tenure regime, Table 9 show reduced-form

does not work through postponement of hard-to-reverse decisions due to risk aversion, but to difficulties in accessing credit markets. For example, Martins and Villanueva (2009) find that sharp changes in the cost of owner-occupied housing do affect the living arrangements of the youth.

estimates of a multinomial logit model, where we consider three different housing tenure regimes: home ownership, rental and coresidence (living in parents' house). The latter is the base reference category. We show reduced-form estimates of the housing tenure regime on regional subsidies instead of providing Instrumental Variable (IV) estimates, because the intention-to-treat estimates are much more precise in a non-linear setting. Moreover, the IV estimates are not well-defined in a multinomial logit setting.

The minimum sample size of our sample of household members aged between 20-40 years is 7,649. To deal with item non-response, the EFF survey provides five data sets imputed multiply in order to take into account the uncertainty about the imputed data. We follow Rubin's rules for combining estimates done in multiply imputed data sets [see Rubin (1987)].

Table 9 shows that the 49.8% of household members being employees and aged between 20 and 40 are the owners of their main dwelling, 15.2% of them rent their housing accommodation and the rest of them (35%) live with their parents. The first column in Table 9 show the Ordinary Least Square (OLS) estimates of the indicator of whether the household member holds a permanent contract on the mean subsidy in the first two years of the worker's job tenure that the firm can benefit from their region for converting a fixed-term contract into an open-ended one. Column (1) shows an increase of 2.2% in the stock of permanent workers when the regional subsidy for the job contract conversion increases in 1,000€. This estimate is statistically significant at the 1% significance level and the F-test associated with the instrument (regional subsidies) in the first-stage estimates of the permanent contract indicator is 15.03. Columns (2) and (3) show that regional subsidies are significant to explain housing tenure regime at the 1% significant level. Household members whose firms are eligible for regional subsidies to convert fixed-term contracts into permanent ones are more likely to be homeowners or tenants of their housing accommodation than to continue living with their parents. The impact on probabilities is a bit higher for the home ownership regime vs rental. An increase of 1000 euro in the subsidy rises the probability that the household member is observed to be a homeowner in 0.010

and the probability of being a tenant in 0.004; these probabilities move from 0.39 to 0.40 in the case of home ownership and from 0.14 to 0.144 in the case of rental. In conclusion, job security identified by the regional subsidies to permanent contract conversions makes more likely to observe household members forming a household independent of their parents than living with them. This stock sample of workers coming from the EFF allows us to study the decision of household formation and housing tenure in the long-run, not so immediately to the signing of the permanent contract, unlike a sample of flows.

6 Conclusions

This study exploits two institutional features of the Spanish labor market to address the question: Does the growing incidence of employment insecurity among young adults account for their recent patterns of household formation? The advantage of working with Spanish labor market data is the widespread use of low-firing cost contracts, that allows us to identify which young adults are exposed to the risk of losing their job. We can also exploit legal changes that influence the labor demand of firms for workers with different contract types to obtain arguably exogenous variation in employment insecurity. Thus, we are able to estimate the link between obtaining a more secure jobs and the decision to establish a new household controlling for other confounding factors.

We use two strategies to identify the causal link between job insecurity and household formation. The first strategy exploits a legal limit between 1988 and 1994 that required firms to convert temporary jobs into permanent ones after three years of continuous relationship between a worker and a firm. Such legal limit creates a discontinuity in the chances of obtaining a more secure job three years after signing a contract, that we use to identify the impact of changes in firing costs on household formation. The second strategy uses temporal and regional variation in subsidies to convert low-firing cost contracts into high firing costs ones between 1998 and 2001. Each strategy identifies the impact using a different group of the population and a different time period, and both consistently lead to the same conclusion; the link between job insecurity and household formation is at best weak. Finally, we provide tentative evidence that in the long-run job security may affect the housing tenure regime of young household members that live independent of their parents.

We would like to flag three lines of research. The first is to embark in a full-fledged study of the impact of changes in the risk of losing the job in the decision to form a household by owning a house or by renting one. The second is to examine the impact of lay-off costs on outcomes like household consumption or portfolio composition. The third is to analyze the link between low-firing costs contracts and household formation in countries other than Spain.

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Table 1: Summary statistics of sample used to analyze the impact of legal time limits.

Panel A: Young coresidents first observed working with a FT contract

	Mean	Standard deviation	Minimum	Maximum
Contract changed from temporary into permanent	0.022	0.147	0	1
Young adult formed a new household	0.049	0.217	0	1
Age	22.67	3.382	20	35
Potential tenure (in years)	**1.51**	**1.614**	**0**	**21.25**
Actual tenure (in years)	0.997	1.487	0	20
Male	0.63	0.483	0	1
Household size in parental hhold.	4.328	1.291	2	13

Sample size: 244,253

Panel B: Young adults whom we observe working in some period

	Mean	Standard deviation	Minimum	Maximum
Job covered by permanent contract	0.517	0.500	0	1
Adult lives with parents	0.603	0.489	0	1
Age	25.058	4.209	20	35
Potential tenure (in years)	**3.39**	**4.312**	**0**	**22**
Actual tenure (in years)	**3.28**	**4.33**	**0**	**20**
Male	0.62	0.485	0	1
Household size in parental hhold.	3.391	1.507	1	13

Sample size: 633,621

Table 2: Sample used for analysis of the impact of subsidies: 1998-2001

	Sample (1), flows			Sample (2), stocks	
	Subsidy	Contract changed from transitory to permanent	New household formed	Permanent contract (1 if permanent, 0 ow.)	Lives with parents
1. Andalucia	1057.22	0.020	0.028	0.355	0.558
2. Aragon	389.25	0.038	0.022	0.499	0.519
3. Asturias	768.38	0.018	0.022	0.474	0.603
4. Baleares	0	0.041	0.028	0.527	0.470
5. Canarias	477.8	0.035	0.023	0.442	0.558
6. Cantabria	732.05	0.017	0.014	0.474	0.659
7. Castilla-Leon	1216.01	0.031	0.025	0.470	0.555
8. Castilla-La Mancha	0	0.031	0.024	0.419	0.542
9. Catalonia	0	0.042	0.024	0.512	0.534
10. Valencia	1289.64	0.034	0.022	0.443	0.519
11. Extremadura	2222.25	0.028	0.027	0.431	0.509
12. Galicia	477.47	0.023	0.022	0.425	0.622
13. Madrid	0	0.04	0.015	0.618	0.601
14. Murcia	1633.22	0.035	0.026	0.409	0.554
15. Navarra	831.51	0.045	0.023	0.506	0.585
16. Basque country	1661.96	0.033	0.022	0.440	0.605
17. Rioja	2681	0.037	0.017	0.506	0.55

Source: Own computations from the 1998-2001 waves of the Encuesta de la Población Activa
1. The first three columns present summary statistics on a sample of working young adults between 20 and 35 years of age, years 1991-1997. whom we first observe living with a parent and working on a job regulated by a temporary contract.
2. Subsidy amounts (in 1995 euros) indicate the amount that the group the young belongs to may qualify for. They do not reflect actual take-up
3. Last two columns correspond to a sample of working young adults between 20 and 35 years of age.

Table 3. Changes in type of contract and household formation.

Dependent variable takes value one if young adult leaves the house of parents, zero otherwise. Estimation method: OLS

	Model I	Model II	Model III
Change from fixed-term to permanent	.0057	.0051	--
	(.0011)	(.0010)	
Contract changed in the present quarter	--	--	.0104
			(.0016)
Contract changed last quarter	--	--	.0074
			(.002)
Contract changed two quarters ago	--	--	.0011
			(.0018)
Age between 20 and 25 years	-.0129	-.0128	-.012
	(.0008)	(.0008)	(.0008)
Age between 31 and 35 years	-.0063	-0.006	-.0062
	(.00012)	(.0012)	(.0012)
Female	.0033	.00339	.0033
	(.0008)	(.0008)	(.0008)
Family size equals 3	.0047	.0046	.0047
	(.0016)	(.0016)	(.0016)
Family size equals 4	.0047	.0047	.0047
	(.0015)	(.0015)	(.0016)
Family size equals 5	.0089	.0088	.0088
	(.0016)	(.0016)	(.0016)
Family size equals 6 or more	.0093	.009	.0092
	(.0017)	(.0017)	(.0017)
Constant	.022	.012	.0218
	(.0024)	(.003)	(.002)
Year dummies	YES	YES	YES
Province dummies?	NO	YES	NO
Observations	198,310		198,310

Notes: 1987-1995 waves of the Spanish Labor Force Survey (Encuesta de Población Activa). Sample of individuals between 18 and 35 of age observed living with their parents and with a temporary contract. Education, industry and region included in Models 1 and 2.
2. Constant is an estimate of the probability of forming a new household of a young adult between 25 and 29 years, who has completed at least high school, lives in a family of 2 persons, works with a temporary contract in the food industry in Andalusia

Table 4. The impact of legal time limits on type of contract

Estimation method: OLS						
Window:	Between .5 and 6.75 years since FTC		Between 1.25 and 4.5 years			
Sample, by age and gender:	20-34	25-34	20-34	25-34	25-34 females	25-34 males
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable takes value 1 if young adult works under a permanent contract, 0 otherwise						
d(3<years since FTC)	.036	.0264	.0429	.043	.043	.0426
	(.014)**	(.013)**	(.011)***	(.012)***	(.015)***	(.021)***
Female	-.0217	-.0244	-.037	-.040		
	(.0032)	(.0036)	(.004)	(.004)		
Years since FTC	-.069	-.0698	.64	.616	.846	.47
	(.197)	(.086)	(.131)	(.082)	(.113)	(.16)
Years since FTC, squared	.143	.134	-.165	-.16	-.288	-.082
	(.041)	(.054)	(.067)	(.043)	(.057)	(.079)
Years since FTC, cubed	-.026	-.023	.0182	.0178	.04	.005
	(.0076)	(.0099)	(.011)	(.007)	(.009)	(.013)
(Years since FTC -3)*d(3<years since FTC)	.1247	.0748	-.123	-.122	-.244	-.051
	(.042)	(.0487)	(.065)	(.067)	(.087)	(.11)
(Years since FTC -3) squared*d(3<years FTC)	.069	.0558	.163	.137	.151	.12
	(.035)	(.0435)	(.081)	(.081)	(.129)	(.14)
(Years since FTC -3) cubed*d(3<years FTC)	.0292	.0253	-.086	-.077	-.132	-.044
	(.0077)	(.010)	(.029)	(.030)	(.053)	(051)
Sample size	379499	242983	171148	111111	39543	71568
test F d=0			16.38	12.84	12.84	4.12
Panel B: Dependent variable: Young adult lives with his or her parents						
d(3<years since FTC)	-.003	-.0014	.000	.003	.0051	.0016
	(.007)	(.0083)	(.001)	(.002)	(.0049)	(.007)
Female	-.065	-.065	-.0108	-.0132		
	(.011)	(.011)	(.0016)	(.0023)		
Sample size	379499	242983	171148	111111	39543	71568

1. d(exp>3) is a binary variable that takes value 1 if more than three years have passed since a temporary contract was signed
3 Standard errors clustered at the level of years elapsed since a fixed-term contract was signed (in quarters).
4. Covariates included in all specifications but not shown: age dummies, industry, and schooling
5. **, *** statistically significant at 5%(1%) confidence level

Table 5. The impact of legal limits on contract conversion 1988-1994

Estimation method: OLS

	Sample used, by gender and age			
	20-34, all	25-34, all	20-34 females	20-34 males
	(1)	(2)	(3)	(4)
Panel A: Dependent variable takes value 1 if temporary contract upgraded into permanent, 0 otherwise				
Aggregated at quarter-since FTC, no covariates				
1. d(3<=years since FTC<=4)	.0517	.0459	.0594	.0466
	(.0128)	(.0238)	(.0124)	(.0141)
Adding covariates (a)				
2. d(3<=years since FTC<=4)	.0548	.0533	.0641	.0491
	(.0061)	(.0169)	(.0088)	(.0101)
+- 1.5 years around the 3-year limit, covariates				
3. d(3<=years since FTC<=4)	.0447	.0427	.0423	.0472
	(.0104)	(.0202)	(.0095)	(.015)
Panel B: Dependent variable: Young adult forms a new household				
1. Aggregated at quarter-since FTC, no covariates				
d(3<=years since FTC<=4)	-.0087	-.005	-.0028	-.0114
	(.0134)	(.0209)	(.0199)	(.0131)
2. Adding covariates (a)				
d(3<=years since FTC<=4)	-.011	-.0054	-.0046	-.0152
	(.0097)	(.0155)	(.0155)	(.0091)
3. Reduced time window, covariates				
d(3<=years since FTC<=4)	-.0054	-.0055	.0009	-.0092
	(.0048)	(.0089)	(.0105)	(.0052)
Observations	40353	17743	16052	26254

Source: EPA, 1988q1-1994q4

1. d(3≥exp≤4) is a binary variable that takes value 1 if between 3 and 4 years have passed since the first time we observe a young adult working under a fixed-term contract.
2. "Years since" is the number of quarters elapsed since the first time we observe the individual working with a fixed-term contract, see text
3. Standard errors allow for arbitrary correlation between observations with the same time since first observed with a fixed-term contract
4. *, ** statistically significant at 5%(1%) confidence level

Table 6: The impact of regional subsidies on contract conversion (fixed-term into permanent)

Estimation method: Probit

Panel A: Dependent variable takes value 1 if temporary contract upgraded into permanent, 0 otherwise

	Age 20-34, all	Age 25-34, all	Age 25-34, males
	(1)	(2)	(3)
Subsidy amount	.0032	.0032	.0032
	(.0012)*	(.0014)*	(.0018)
Male	.003	.0007	--
	(.0032)	(.0040)	--
Age between 20 and 24	-.0098	--	--
	(.0025)*	--	--
Age between 30 and 34	-.0102	-.0092	
	(.0040)*	(.0038)*	
Household size of 3	.0055	.0004	-.0009
	(.0028)	(.0044)	(.0053)
Household size of 4	-.0064	-.005	-.0058
	(.0038)	(.004)	(.0049)
Household size above 5	-.0064	-.005	-.0023
	(.0039)	(.004)	(.0056)

Panel B: Dependent variable takes value 1 if young adult forms a new household, 0 otherwise

Subsidy amount	-.0003	-.0005	-.0002
	(.0004)	(.0005)	(.0006)

(rest of covariates are the same as in Panel A, but not shown)

1. Standard errors allow for arbitrary correlation between observations belonging to the same region and age group
2. * statistically significant at 5% confidence level or above
3. Additional covariates: region dummies, gender, occupation, industry, schooling, and separate year and quarter dummies

Table 7: The impact of contract conversion on household formation

Dependent variable takes value 1 if young adults forms a new household in the quarter, 0 otherwise

Sample, by age and gender:	20-24, all	25-34, all	25-34 males
1. Change fixed-term contract - permanent contract	-.0288	-0.0218	-0.0181
(marginal effect for adult 25-29, see footnote)	(.0124)	(.057)	(.048)
2. Change fixed-term contract - permanent contract	-1.2670	-.9548	-0.7235
	(.1871)	(.7121)	(.525)
3. Male	-.0397	-.0271	--
	(.0172)*	(.0267)	
4. Current regional unempl. rate	.0061	.0061	.0086
	(.0039)	(.0039)	(.0039)
5. Age 20-24	-.2316	--	--
	(.0172)**		
6. Age 30-34	-.1182	-.1122	-.1146
	(.0258)	(.0249)	(.0369)
7. Household size equals 3	0.1591	.1588	.159
	(.002)	(.0283)	(.0388)
8. Household size equals 4	.0826	.0823	.0975
	(.0195)	(.02303)	(.0313)
9. Household size equals 5 or more	.1586	.1955	.2021
	(.0222)	(.0290)	(.0285)
Correlation unobservables in outcome and selection	.5472	0.2842	0.1308
Sample size	141326	79463	48672

Spanish Labor Force Survey (EPA): 1998-2001

1. Estimation method: bivariate probit. Additional covariates (not shown) are region dummies, education dummies, industry dummies

The amount of the subsidy in the region is only included in the selection equation. All coefficients (but those of row 1) shown are those of the index of the bivariate probit with standard errors in parentheses.

2. The coefficients in row 1 are the impact of leaving the parental household for a female between 25 and 29 years of age, living in Madrid, working in the retailing sector and residing in a household in a family of size 2. Standard errors (in parentheses) is computed by using bootstrap (50 replications) where each bootstrap sample preserves the sample structure based on age, gender and region group.

Table 8: Forms of household formation, by type of labor contract

	New Household formation	New household formation:		
		Own house of residence	Rent house of residence	Free-rent
	(1)	(2)	(3)	(4)
Panel A: Spain				
Perm. contract	.077	.053	.016	.007
FT contract	.065	.035	.021	.009
Panel B: Italy				
Perm. contract	.066	.036	.018	.010
FT contract	.067	.032	.021	.013
Panel C: France				
Perm. contract	.110	.018	.087	.004
FT contract	.132	.013	.107	.012
Panel D: Portugal				
Perm. contract	.068	.037	.016	.015
FT contract	.059	.030	.015	.014

Own computations using the 1994-2001 waves of the European Community Household Panel

Table 9: The effect of subsidies for job contract conversions on the stock of open-ended contracts and on the decision of housing tenure during period 2002-2012.

Estimation method:	OLS estimates	Multinomial logit estimates (base outcome coresidence)	
	Open-ended contract	Home ownership	Rental
	(1)	(2)	(3)
1. Subsidy to contract conversion	0.022	0.055	0.055
(standard error)	-0.006	(0.012)***	(0.016)***
2. Constant	0.140	0.528	0.293
(standard error)	(0.054)	(0.212)***	(0.332)
3. Marginal impact of 1000 euro subs on outcome in the column	--	[0.010]	[0.004]
4. Unweighted probability of outcome in the column	--	0.39	0.14
5. Weighted sample means (%):	--	49.77	15.19
Minimum sample size	7,649	7,649	

Source: The sample is formed by all household members aged between 20 and 40 years, who are employees and who have been interviewed in the 2002-2011 waves of the Spanish Survey of Household Finances (EFF). The interviews ended up in 2012.

In columns (2) and (3), cells contain estimates of the latent index coefficient of the outcome in the column, its standard error associated (in parentheses) and the marginal effect of a 1000-euro increase in the subsidy (in brackets). The marginal effects are constructed as the average marginal effect predicted for each household member of the sample. Unweighted probabilities are computed as the average probability fitted for each household member in the sample.

Other covariates included in the model not shown here are the following: indicators of age at hire, indicators of year at hire, year dummies, indicators of the household member's education level, the logarithm of the household member's labor income, the number of years contributed to Social Security and region dummies. Standard errors are corrected for heteroscedasticity and combined across 5 implicates. In column (1), the standard errors also take into account arbitrary autocorrelation among regions.

Table A.1: Minimum amd maximum duration of temporary contracts, by contract type

	1984		1992		1994		1997	
	minimum	maximum	minimum	maximum	minimum	maximum	minimum	maximum
1. Contract to promote employment	6 months	3 years	12 months	3 years	6 months	3 years	--- (contract disappeared)	
2. Practice contract	6 months	3 years	6 months	3 years	6 months	2 years	6 months	2 years
3. Apprenticeship contract	6 months	3 years	6 months	3 years	6 months if age>16 & age<25	3 years	6 months if age>16 & age<21	3 years

1. "Apprenticeship contract" (contrato de aprendizaje): contract typically offered to low-skilled young workers
2. "Learning contract" (contrato de formación): contract typically used for workers between 16 and 18 years of age.
3. "Practice contract" (contrato en prácticas) Contract typically used for qualified young workers without labor market experience

Table A.2: Subsidies for conversion of temporary contracts into permanent ones, by region and year

Region / Year	1997	1998	1999	2000	2001
1. Andalucia			All years, 1,800 euro if age < 30		
2. Aragon			All years, 1,200 euro for females		
3. Asturias	2,100 euro	2,100 euro, all workers	2,100 euro, all workers	2,100 euro, all workers	None
		2,400 if "learning contract"	2,400 euro if "learning contract"	2,400 if "learning contract"	
		600 extra if female in male job	600 extra if female in male job	plus 600 if female in male job	
4. Baleares			None		
5. Canarias	None	3,600 if age<25 or if female	None	None	None
6. Cantabria	None	1,800	None	None	None
		2,400 if age<30 or female			
		3,600 if above 40			
7. Castilla-Leon	None	1,800 euro	1,800 euro	1,803 if age<30	1,803 if age<30
		2,400 if apprenticeship contract	2,400 if apprenticeship contract		2,040 if female
8. Castilla-La Mancha			None		
9. Catalonia			None		
10. Valencia	None	None	30% of payroll tax	30% of payroll tax	1400, practice contr.
					1,800 if "practice c."
					and female
11. Extremadura	4908	3545	3618	2100 if training	2101 if "practice c."
12. Galicia	None	3000 euro if age<30	None	None	None
		4200 if female in male job	None	None	None
13. Madrid			None		
14. Murcia	1800	2100 if age<=30	2100 if age<=30	2100 if age<=30	2100 if age<=30
	2400 if age<30	1500 if age>30	1800 if age>30	1800 if age>30	1800 if age>30
15. Navarra	None	1800	None	Payroll subsidy depending on age	
16. Basque country	None	3000 for age<40	3000 for age<40	Both years: Former+ 6009 euro if age<30	
	None	150 extra if female	150 extra if female	Former+ 4507 euro if age<30 & female	
17. Rioja	None	Depends on # conversions	Depends on # conversions	Depends on # conversions	

1. "Apprenticeship contract" (contrato de aprendizaje): contract typically offered to low-skilled young workers
2. "Learning contract" (contrato de formación): contract typically used for workers between 16 and 18 years of age.
3. "Practice contract" (contrato en prácticas) Contract typically used for qualified young workers without labor market experience

Table A.3. Do1998 subsidies explain pre-1998 contract upgrades?

Dependent variable takes value 1 if contract changed from fixed-term into permanent		
Estimation method:	OLS	PROBIT
Subsidy	0.0009	0.0082
	(0.0008)	(0.0075)
Male	-0.0014	-0.0097
	(0.0023)	(0.0189)
Current regional unempl. rate	-0.0005	-0.0039
	(0.0001)**	(0.0009)**
1997 regional unempl. rate	-2.2056	-22.0520
	(0.3911)**	(5.7145)**
Age	0.0015	0.0129
	(0.0002)**	(0.0021)**
Household size	0.0012	0.0109
	(0.0006)*	(0.0052)*
Regional housing cost	0.2543	2.5209
	(0.04503)**	(0.6577)**
# Young adults	51203	
Observations	187768	

Coefficients in the second column denote coefficients from the Probit index.

Figure 1:
Living arrangements and type of contract among Spanish youth: 1987-2004
sub-title

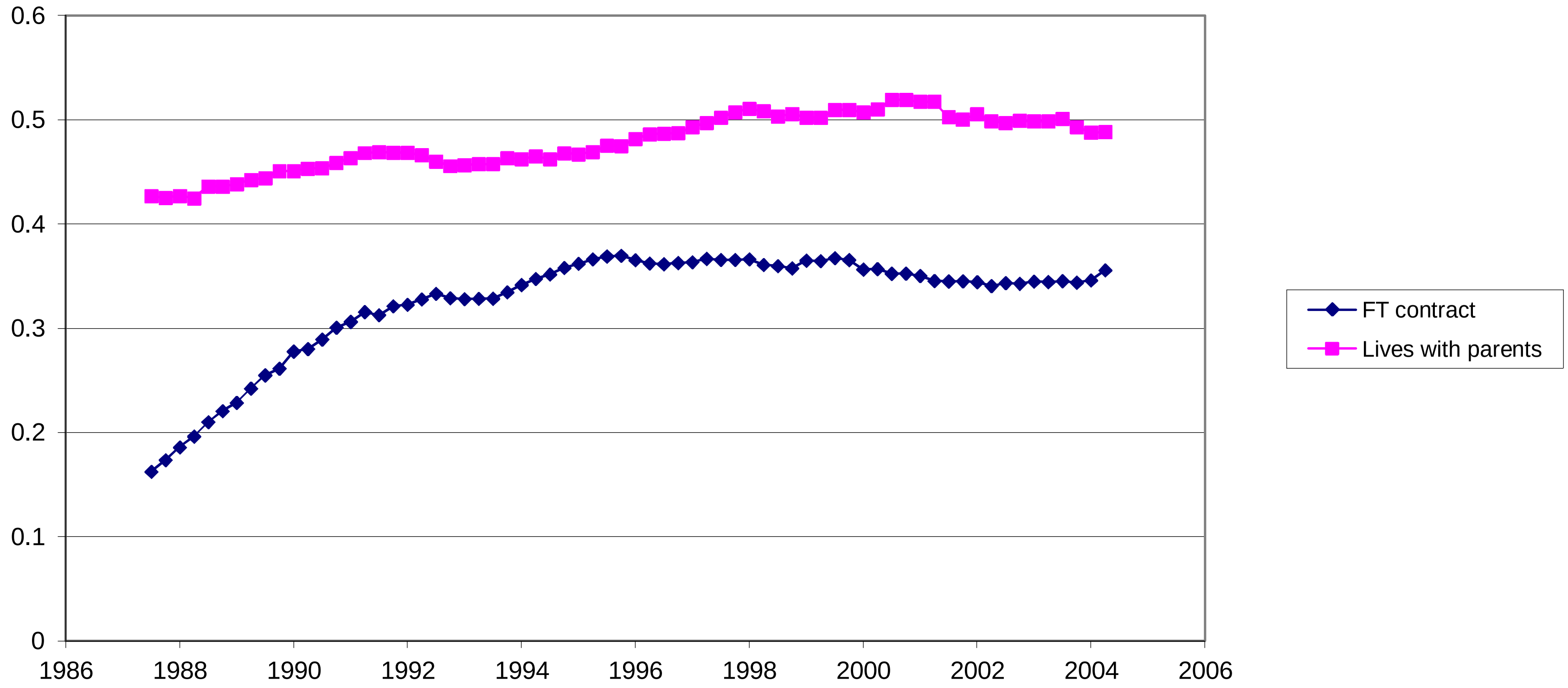


Figure 2: Fraction of youth with high firing cost contract: 1988-1994

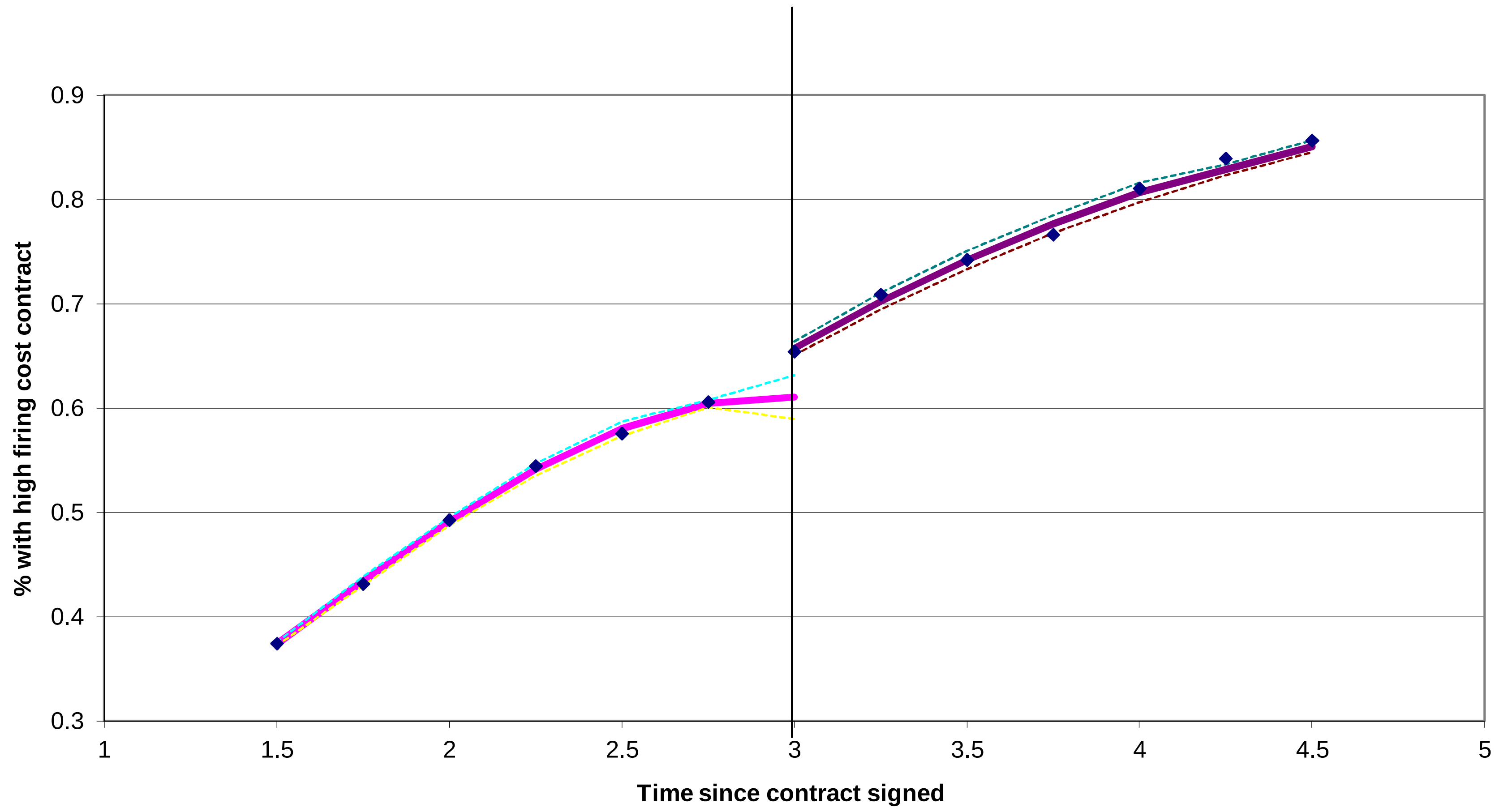
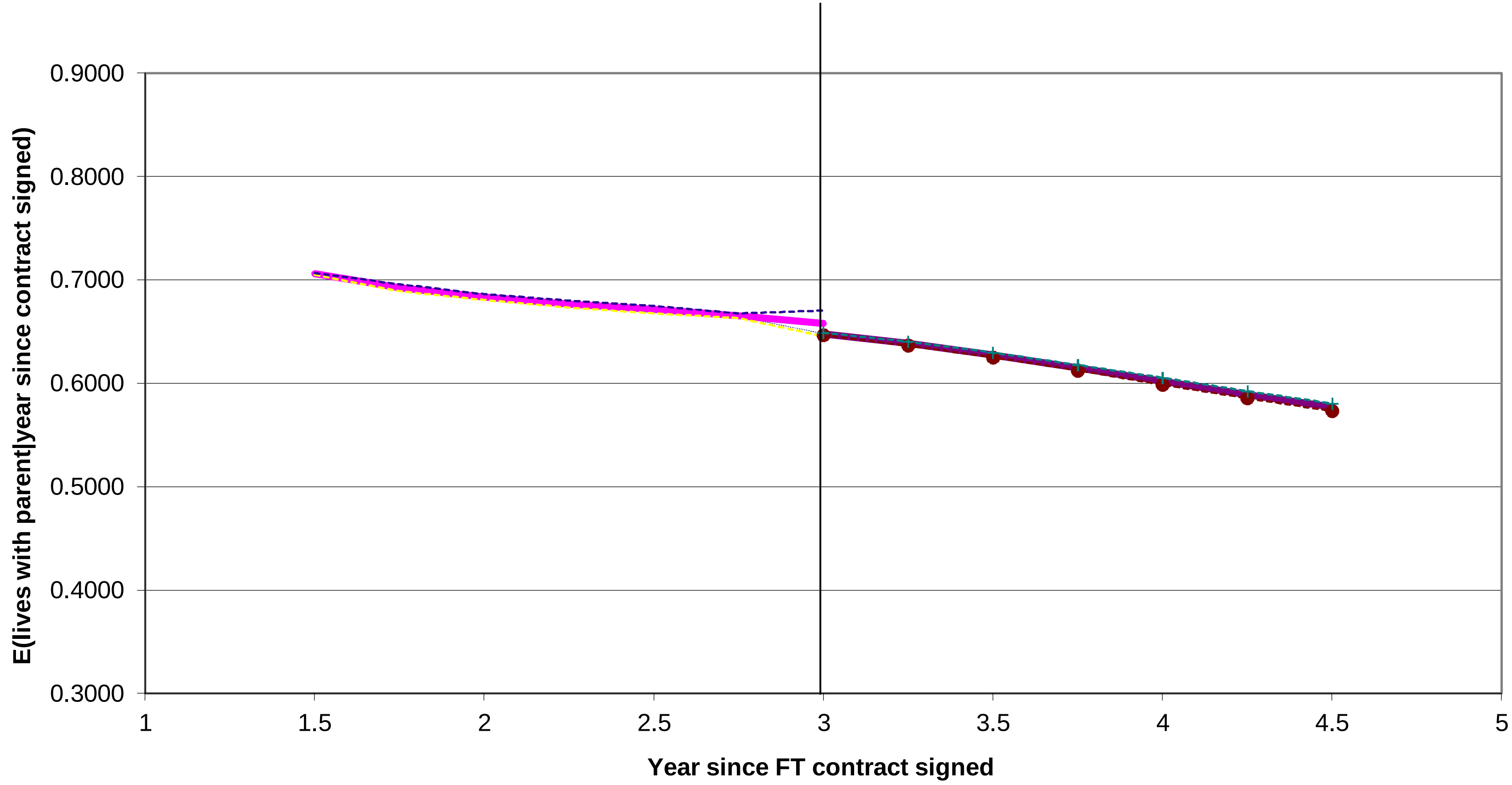
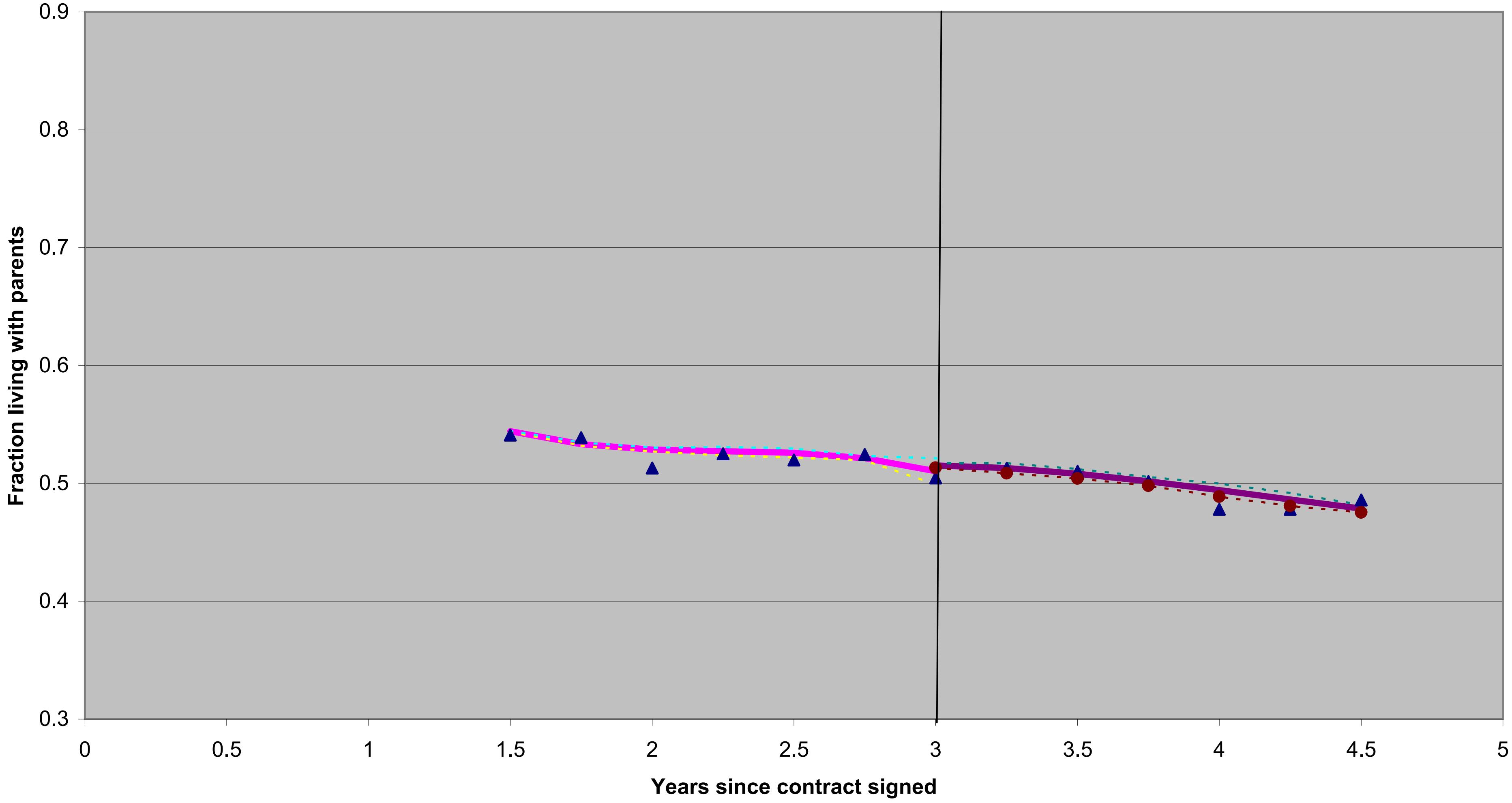


Figure 3:
Fraction of youth between 20 and 34 years living with parents, by year since contract signed: 1988-1994



Graph 4: Living arrangements og young adults 25-34 years of age, by year since contract signed: 1988-1994



Graph 5: Conversion rates by year since FT signed: 1987-1994

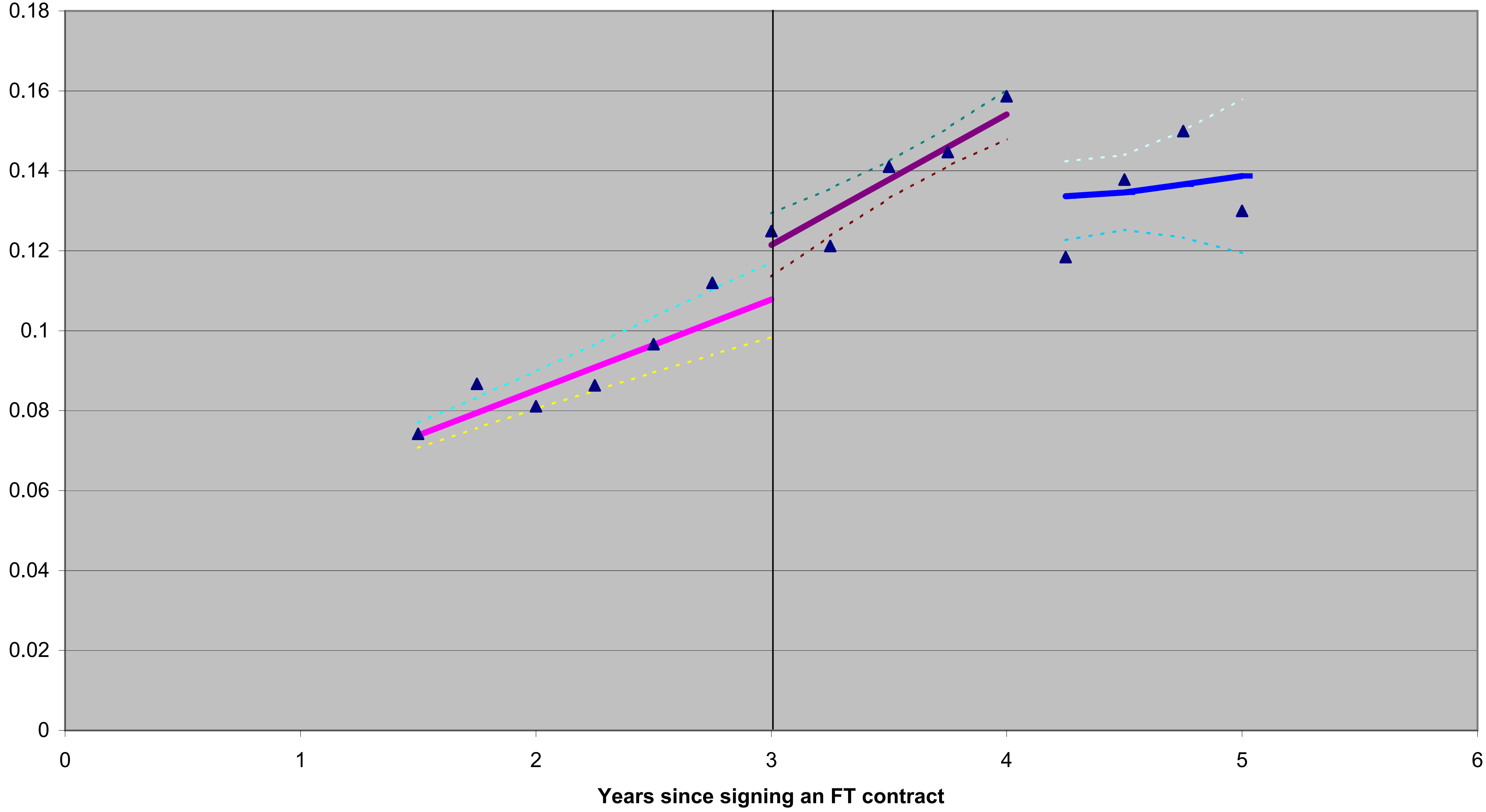


Figure 6: Contract conversion rates, by year since FTC signed: 1995-1998

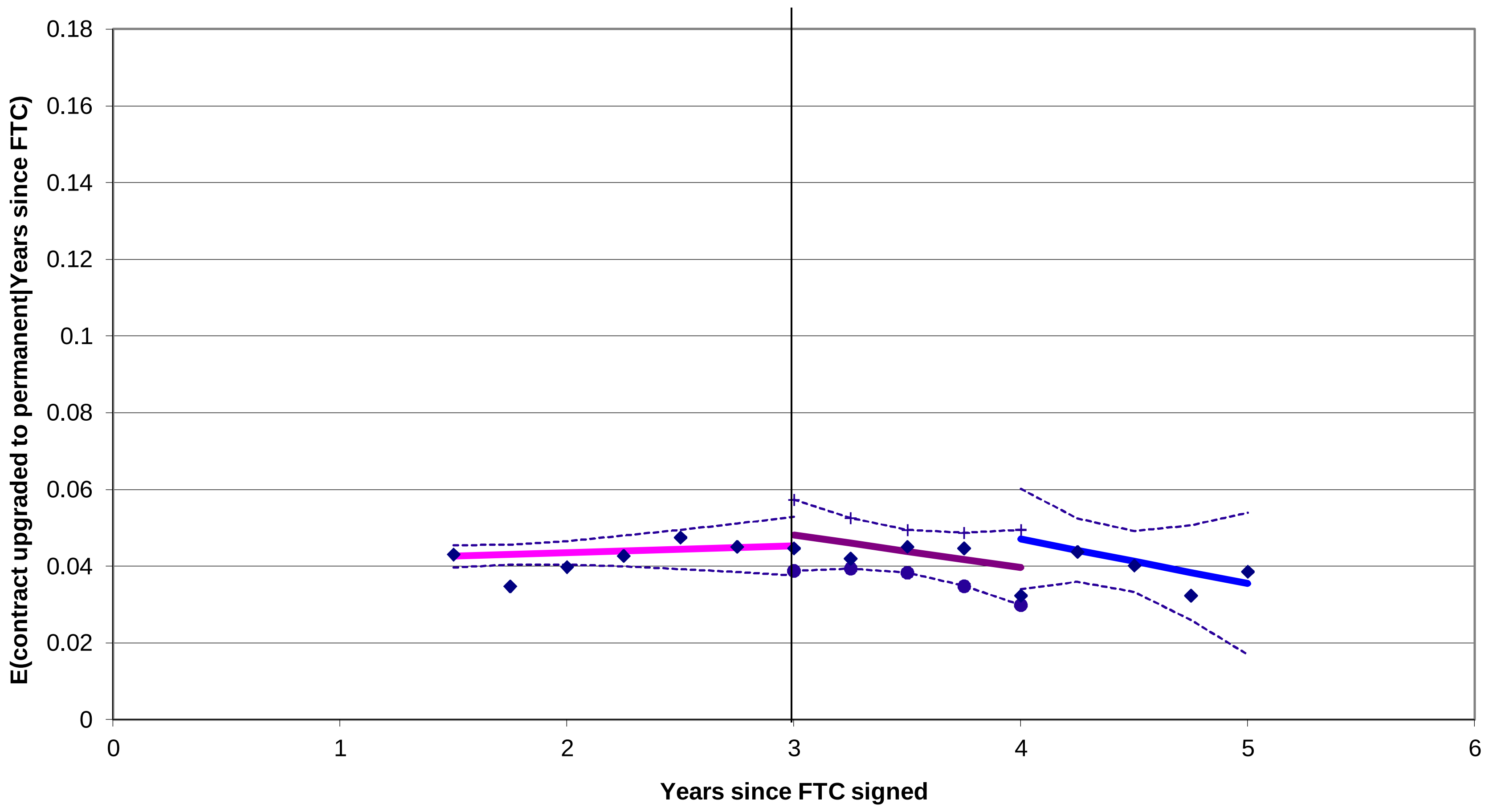


Figure 7: Contract change and household formation: residuals from a linear regression on second order polynomial of time since FTC signed: 1988-1994

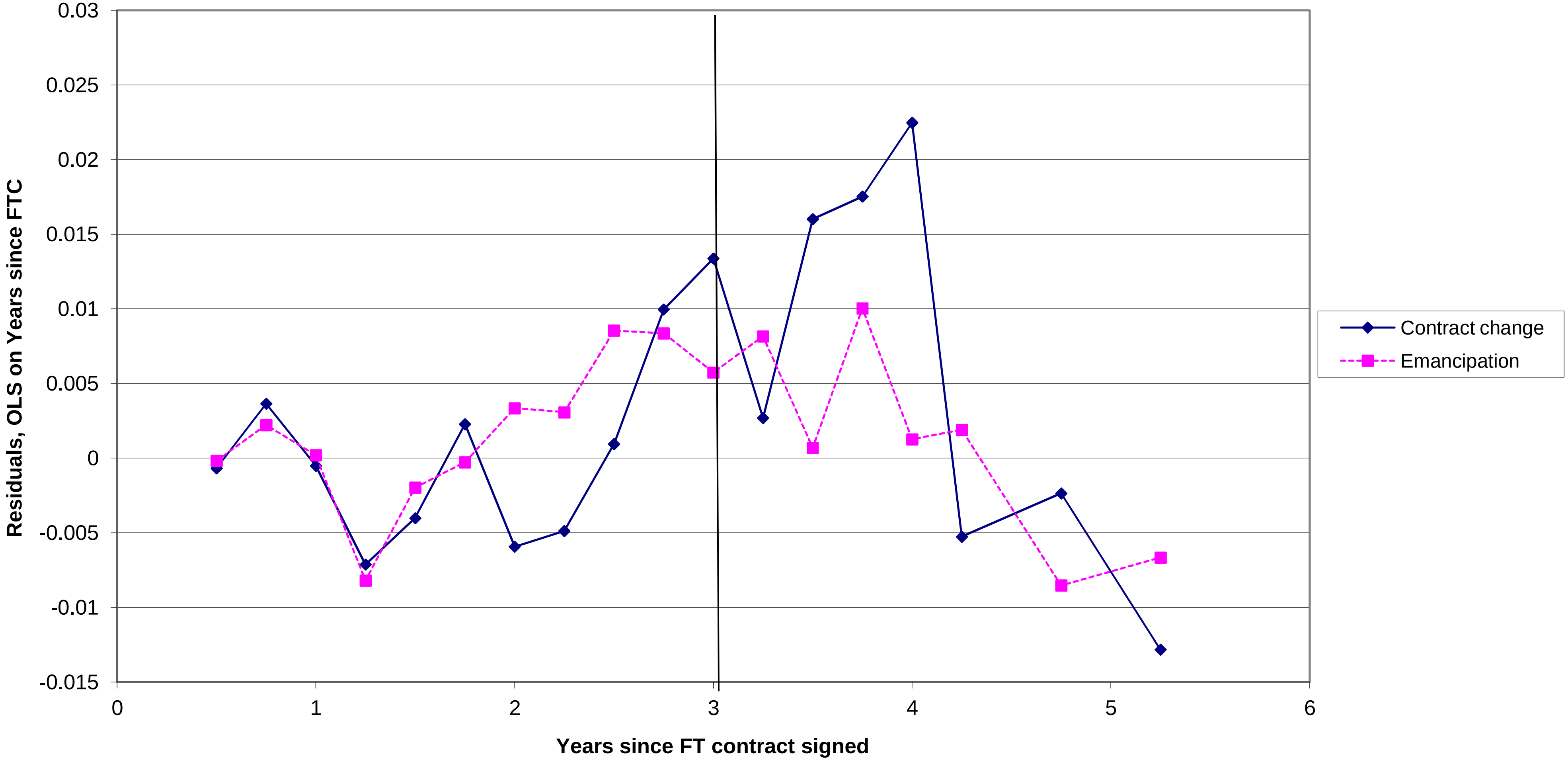


Figure A.1 One-year change in satisfaction with job security
sub-title